Reply to Reviewer Michael Montgomery of "The vorticity budget of developing Typhoon Nuri (2008)" by D. J. Raymond and C. López Carrillo

September 21, 2010

Reviewer's comments are inset. Replies are full width.

> DR. Montgomery
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> Received and published: 30 August 2010
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> Review of ACPD manuscript:
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> The Vorticity Budget of Developing Typhoon Nuri (2008) by D. Raymond and C. Lopez-Carillo
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> This paper examines the formation and intensification of typhoon Nuri and the non-development of a tropical disturbance observed near Guam in the western North Pacific during the 2008 typhoon season. Both of these tropical disturbances were observed as part of the United States, Office of Naval Research sponsored Tropical Cyclone Structure 2008 field experiment conducted out of the island of Guam and coordinated by the Department of Meteorology at the U.S. Naval Postgraduate School, in Monterey, California.
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> These two tropical disturbances were examined with U.S. Air Force C-130J and Naval Research Laboratory WP3D aircraft and personnel. Like the C-130J's, the P3 aircraft flew with a full instrument suite, which included the Doppler Radar (EDLORA) and Wind Lidar.
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> The mesoscale wind analyses derived from the dropsonde instruments released from both aircraft and the ELDORA radar system complement a related study by other authors (i.e., Montgomery, Lussier, Moore and Wang 2010, ACP) examining the synoptic and sub-synoptic aspects of the formation of typhoon Nuri. I believe the results presented here represent a significant contribution in the study of typhoon/hurricane formation from easterly wave precursors. I think that this paper has the potential of becoming a landmark investigation. That being said, I have several concerns with the presentation and interpretation that require careful consideration and revision before I can recommend publication in ACP.

Major comments:

1. P.2: The discussion and implications of the (nonlinear) Ekman balance approximation in the manuscript needs considerable improvement so that graduate students and more established researchers can understand the implication of the results. The authors' results indicate that the Ekman balance approximation is highly inaccurate. Based on some of my collaborative work with Roger Smith and colleagues, I am not at all surprised by this particular finding! The authors have not yet provided a convincing argument that this Ekman balance is relevant to the analyses carried out. See point 3 below for more.

In response to Roger Smith's comments we have completely redone the theory section of this paper. We agree that Ekman balance is not relevant and have removed all reference to it.

2. Smith and Montgomery (2008 QJRMS: SM08) refer to this type of approximation as a "balanced boundary layer". In that paper the authors presented a novel scale analysis showing that this approximation cannot be justified a priori in TC vortices and pre-TC circulations when the mean vortex Rossby number, based on the absolute vertical vorticity of the mean vortex, is not small compared to unity. In particular, SM08 demonstrated that such a balanced boundary layer is highly inaccurate as a boundary layer model for a TC vortex. However, the current presentation appears to give readers the impression that this issue will be overcome here by using the vorticity equation in flux form:

"This issue is finessed here by considering not the primitive equations directly, but the vorticity equation"

and the subsequent sentence

"This vorticity balance is related to, but is not identical to Ekman balance, as it incorporates advection terms from the momentum equation in addition to the Coriolis force and friction terms."

My question is this: Is the analysis presented here fundamentally different from the balance approximation examined in SM08? While it is certainly true that the full primitive equations contain everything that the vorticity equation does (in either flux or material form), the converse is not true. Namely, the vorticity equation is not a complete description of the boundary layer dynamics of a tropical cyclone vortex! Either the radial momentum equation or its equivalent is necessary to complete the analysis and interpretation.

Three points: (1) Vorticity balance is exact except for the steady state assumption, so in fact it is not equivalent to any previous balance condition. Therefore it is valid for any steady boundary layer. Furthermore, the type of "balance" here is fundamentally different than that represented by Ekman balance and friends to which the reviewer refers. This became abundantly clear after consulting with Roger Smith. (2) The reviewer is correct that vorticity balance is not a complete description of the boundary layer. The divergence equation is indeed needed to complete the dynamical description. (3) The vorticity equation (and vorticity balance in the steady state) is sufficient for the diagnostic use made of it in this paper, since we can measure or estimate all the terms in the vorticity balance condition. Furthermore, the degree of imbalance allows us to estimate the vorticity tendency at the time of the observation. The divergence equation is discussed in the new theory section, but it is not used. (It could be if it were desired to diagnose the pressure perturbation field. However, we have not as yet undertaken to do this.)

> 3. Since the present study is focused on the spin up dynamics of typhoon Nuri, and in view of the findings by SM08, the authors are encouraged to use their analysis to evaluate the accuracy of Emanuel's (1997) the time-dependent WISHE model for tropical-cyclone intensification and the underlying 'balanced boundary layer' approximation used therein (the unnumbered equation above Emanuel's Equation (14)). In Emanuel's time-dependent WISHE model, the radial velocity in the boundary layer is obtained from the tangential (sic.) momentum equation, much like your vorticity balance approximation!

Emanuel's model applies to a strong tropical storm where an eyewall has already formed if I read his paper correctly. Our observations apply to a much earlier phase of storm development. Furthermore, so much of the imbalance in the boundary layer in our case is likely due to non-steady behavior that the type of imbalance to which the reviewer refers is likely to be obscured.

> 4. If your point is to show that 'Ekman balance' cannot by itself spin up the winds in and near the boundary layer of typhoon Nuri, then I agree wholeheartedly. A comparison of the tangential wind tendencies was carried out in Bui et al. (2009) using the full primitive equations and using the balanced boundary-layer approximation. The balanced boundary-layer approximation was shown to be a very poor approximation for capturing the spin of the tangential winds in the boundary and near its top.

We now cite the Bui et al. paper.

> The radial gradient of the local buoyancy and the induced convergence of vorticity in the boundary layer [associated with the deep VHTs that you are documenting here] is needed. From the perspective of the axisymmetric Sawyer-Eliassen balance model, this process requires a sufficiently negative radial buoyancy gradient to offset the divergence above the boundary layer caused by the frictional convergence in the boundary layer below. (The paper by Montgomery et al. (2006, Sec. 7b-7c.) provides additional evidence during the genesis stage.) I think this is, indeed, what is happening.

We think another way of saying this is that there has to be sufficient upward convective mass flux associated with the positive buoyancy anomaly to take up the mass expelled from the boundary layer by frictional convergence in order to avoid spindown. We believe that vorticity balance in the boundary layer is an alternative way of viewing this process. If the actual convergence exceeds the frictional convergence, spinup occurs, if not, spindown occurs.

5. P 2: “However, the development of strong near-surface vorticity is necessary for the amplification of the cyclone vortex and the initiation of the cyclone heat engine (Emanuel,1986).”

Although this sounds like a reasonable summary at first blush, are you advocating that the intensification of Nuri as analyzed in your work occurs along the lines of Emanuel's AXISYMMETRIC heat engine model (WISHE)? (Emanuel 1989, 1995, 1997, 2003)? Strictly speaking, as far as we are aware, Emanuel does not have an ASYMMETRIC heat engine model for tropical cyclone intensification.

Your results suggest that the intensification process occurs via the VHT pathway of Nguyen et al. (2008, QJRMS) and Montgomery et al. (2009, QJRMS) and not the WISHE pathway.

I suggest strongly that the theme of this paper be refocused more on this aspect (post genesis) rather than the non-applicability of Ekman balance.

We have removed this somewhat contentious statement from the paper. The whole thermodynamic aspect of what is going on will be addressed in the next paper. All reference to Ekman balance has been removed and the paper is focused on 4 topics of interest, only one of which is the degree to which vorticity balance holds in and out of the boundary layer. An argument is also made which quantifies whether the steady state assumption behind vorticity balance is approximately justified.

References cited:

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Montgomery, M. T., V. S. Nguyen, J. Persing, and R. K. Smith The genesis of Typhoon Nuri as observed during the Tropical Cyclone Structure 2008 (TCS-08) field experiment. Part 1: The role of the easterly wave critical layer, Atmospheric Chemistry and Physics, in press: 2009,

M. T. Montgomery, L. L. Lussier III, R. W. Moore, and Z. W. Wang